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## 2 Fifty Years in Social Psychology: Some Reflections on the Individual-Group Problem

Harold H. Kelley

Harold Kelley's contributions to the field of social psychology are both broad and profound. In this chapter he uses the metaphor of reflections to argue that the field stands between two counterimposed mirrors—the mirror of the individual and that of the group. How we look into these mirrors illustrates four ways of phrasing the subject matter of social psychology: the individual or the group, the individual versus the group, the individual from the group, and the individual against the group. Kelley describes how social psychology and his own research have navigated through these four reflections. He begins with his experiences at Yale, where, along with several other notable social psychologists, he helped bring a group focus to the study of mass communication processes. He emphasizes his close and long-lasting association with John Thibaut and their numerous works on interdependence. Kelley regards his work on attribution—most notably the development of the famous Kelley cube—as an example of the tradition of research on the individual versus the group. He tells how his 1978 book with Thibaut, on interdependence patterns and how they may define and shape individual differences, is in the tradition of the approach emphasizing the individual from the group. Kelley shows how some of his studies at Yale, as well as his ANOVA model, are examples of how individuals may resist group influences.

My reflections are not scholarly, researched conclusions. They're the ways I remember things and feel about them now. This egocentric approach seemed to be the surest way to avoid redundancy with the other authors of these reflections.

"Reflections" is an apt metaphor for some of the major themes of social psychology. Located as it is between the social and the psychological, we can think of social psychology as standing between two counterposed mirrors, the one the mirror of the individual and the second the mirror of the group. We can look into the one, or we can look into the other. But when we look into either one, we



see reflections from the other, including its reflections of the mirror we're viewing directly.

Using that metaphor (and with apologies to Charles Horton Cooley), I've located my comments under four counterposings of the individual and the group:

1. The individual *or* the group? (What is the proper focus of our field? Which mirror do we look directly into?)
2. The individual *versus* the group? (The "person" versus the "interpersonal situation" as causes of behavior? When we see behavior, in which mirror does its image originate?)
3. The individual *from* the group? (How are individual differences related to or derived from the group? When we look into the individual mirror, what are we seeing from the other side?)
4. The individual *against* the group? When are individuals independent of the group? When are images in the individual mirror independent of those in the other?

Those four themes have been important in social psychology over the twentieth century and are, to varying degrees, reflected in my own fifty years of work. In discussing them, I include a few stories along the way. These are my recollections about why certain things went as they did during those years.

First a few comments about my background. Unlike the other authors in this book, I came from a rural area. My father was a farmer in the small town of Delano, California, located some 90 miles south of Fresno. Like most college-bound youngsters there, from Delano High School I went on to Bakersfield Junior College and then to "Cal," that is, Berkeley. I can no longer reconstruct exactly why I became a psychology major, but I did well in my studies. On graduating with a master's degree in 1943, I had the good fortune to go directly into the Aviation Psychology Program of the U.S. Army Air Force. There I worked under the direction of Stuart Cook ("Captain Cook" in those days), developing selection tests and analyzing how various aircrew members did their jobs (e.g., landing a plane, interpreting airborne radar signals).

Heider (1983) concludes his autobiography with a reference to a "friendly spirit" that arranged the sequence of events in which fortune was so kind to him. I resonate strongly to Heider's comment because it applies to my life as well. Surely the relationship with Stuart Cook was the work of such a spirit. I came to trust his judgment fully, and it was on his strong urging that, at the end of the war, I enrolled in the group psychology program at MIT. That decision landed me in what became one of the most influential groups of social psychologists and gave me a head start on a productive career in the field.

Now, let me return to my four "reflection" themes.

## The Individual or the Group

This concerns the basic question of what is the proper subject matter of "social psychology." At MIT we were taught that a group is more than the sum of its parts. Our version of social psychology was focused on "dynamic wholes," closely interconnected—that is, interdependent—sets of individuals. By virtue of their past or their ongoing interaction, they have complex and dense ties: linkages via communication networks, influence via sociometric and status positions, and so on. That focus contrasted with that of earlier social psychologists who argued that only individuals are real and a group is no more than the sum of individuals' actions.

In research the individual focus was illustrated by Triplett's and similar work that examined such things as the effect of observers on a person's activities. In this tradition experiments usually used strangers as subjects, and the interaction was highly constrained and, generally, brief.

The group focus is well illustrated by the leadership studies at Iowa; by Festinger, Schachter, and Kurt Back's study of a housing project at MIT; by Deutsch's study of contrasting classroom incentive systems; and by Thibaut's laboratory study using gangs from Boston neighborhoods. (My own work on first impressions wasn't in that vein, though, like Mort Deutsch and several others, I received a Ph.D. from MIT in *group* psychology.) The studies were marked by the use of ongoing groups and by the effort put into documenting the processes within those groups. That stands in contrast to the individually oriented work with strangers, short time spans, and concern with end products rather than process.

From 1950 to 1955, my role at Yale, in Carl Hovland's program, was to bring a "group" focus to bear on mass communication processes. That was in contrast to Hovland's orientation (with its learning-theoretic focus on the individual's comprehension, learning, and retention of information) and Irving Janis's similar individualistic focus on personality and psychodynamics. The group and individual orientations were never brought into confrontation. They existed side by side, which reflected Carl's open personality and the value he attached to eclecticism.

In the late 1940s and the 1950s, the major focus was the "group" one, set in place mainly by the group dynamics people. But then, I think it is clear, the group focus began to blur and was gradually pretty much replaced by the individual focus. This shift occasioned Ivan Steiner's famous question in 1974: "Whatever happened to the group in social psychology?" In this shift, much of the study of groups has been left to neighboring disciplines (sociology, communication, education, management, etc.).

That shift leads me to think that the "group" focus in social psychology does not afford a stable intellectual orientation for psychologists. To mix metaphors a bit, it is not a firm place for us to stand. Ned Jones wrote that "in a curious way, social psychology has always been ambivalent about the study of groups per se"



(Jones, 1985, p. 77). I offer two possible reasons for this instability of our attention to groups:

1. An institutional reason concerns the relative prestige of various locations in the scientific hierarchy. In the reductionistic aspects of our shift toward the individual, perhaps there is a seeking of hard-science legitimacy and prestige—a disengagement from the softer (perhaps, the more sociological) parts of social psychology and an identification with the physical and biological sciences.

2. The other reason may be found in the problem that Bob Cohen identified as “bubbe psychology,” the natural desire to try to surprise and impress one’s bubbe (Yiddish for “grandmother”) and one’s colleagues. I would argue (as I have in Kelley, 1992) that avoiding the commonplace or “obvious” takes us in one or both directions away from the intermediate level of observable behavior in groups—either to more micro (reductionistic) levels or to more macro (collective, cultural) levels. Leon Festinger’s scientific career might be examined in these terms, with his moves away from groups, first downward to microlevels of individual motivation, then later to motion of the eyeball, and finally upward to the macrolevels of paleontology and history.

In the Thibaut and Kelley collaboration that began in 1953, John and I *did* achieve a stable focus on phenomena at the group level. We did so by hitting upon a comprehensive and systematic theory, the elements of which others might regard as mundane but the combinatorial structure of which brings order to numerous interpersonal and intergroup phenomena.

So another friendly spirit story concerns the Thibaut and Kelley collaboration and how it came about. In 1952 Gardner Lindzey wrote me at Yale, asking me to write a chapter on groups for the new *Handbook* he was editing and suggesting that I ask Irv Janis to be a coauthor. I posed the question to Irv, and he declined. I then asked Thibaut (who had been a fellow graduate student at MIT), and he accepted. John and I found that we greatly enjoyed working together, our minds and temperaments meshed well, and we produced a chapter of which we were rather proud. On the merits of that chapter (at least in part), we were invited to the Ford Center (the Center for Advanced Studies in the Behavioral Sciences, or CASBS, in Stanford) as a team. We intended to write a textbook on groups, perhaps along the line of Homans’s *Human Group*. But we got sidetracked into economic models (I remember drawing numerous indifference curves on the blackboard), and then got caught up with payoff matrices. Luce and Raiffa had just written their survey of game theory, and we studied a draft copy then available at the center. Our book turned out to be a theoretical work that in its use of outcome matrices (in a more relaxed way than the payoff matrices are used in game theory) provided a strongly analytic and organizing approach to group interdependence—an approach we eventually came to call “interdependence theory.”

Our collaboration was importantly determined by some good luck and helpful accidents—Janis’s other competing tasks; the formation of the Ford Center at that time, which gave us the year to work together; the Luce and Raiffa manu-

script; and so on. The Thibaut and Kelley collaboration surely had the benefit of arrangements by one of Heider’s friendly spirits.

That collaboration continued until John’s death in 1986. Further developments in our theory are described below. Over the years, we continued our joint theoretical work, but our respective lines of empirical work diverged rather markedly. I became increasingly obsessed with the dyad, which I felt I could eventually master intellectually. Reflecting his longtime interests in moral and political philosophy, John’s work increasingly consisted of experimental studies of social organizations, norms, and processes. Prominent among those was his brilliant work, with Laurens Walker (a colleague from the University of North Carolina Law School), on procedural justice. I have no doubt that Thibaut was the single most important intellectual influence on my career and work in social psychology.

### The Individual Versus the Group (as Causes of Behavior)

This second counterposing of individual and group refers to the “person versus situation” attribution problem. The “situation” almost always involves one or more other persons, so the “person-situation” contrast is a special case of the individual-group contrast. Is an observed behavior due to the “person,” or is it due to the “group,” that is, due to pressures from other persons? This, of course, is one of the central questions raised by the attribution perspective in social psychology.

My role in the development of attribution theory was that of bringing together under one tent a number of lines of prior and ongoing work. So a brief story about why and how the Kelley cube came about: At Minnesota we “social relations” people (Stan Schachter, Ben Willerman, Ken Ring, John Arrowood, Jerry Singer, Ladd Wheeler, and others) had read and discussed Heider’s book, and I had reviewed it for *Contemporary Psychology*. I had long been a fan of Thibaut and Henry Riecken’s paper on perception of conformity to requests for help from more and less powerful people, and then at the University of California at Los Angeles (UCLA), Ken Ring and Arie Kruglanski had done doctoral theses taking off from some of Thibaut’s work. From Minnesota days, I was familiar with Schachter’s arousal-affect work, which lent itself to attributional interpretations. I had studied “From Acts to Dispositions,” by Jones and Davis (1965). And I interacted at UCLA with Melvin Seeman, a sociology colleague steeped in Rotter’s locus of control ideas. So my head and notes were full of causal perception—and attribution-related stuff. Then came the invitation to write a paper for the Nebraska Symposium on Motivation, and I did the obvious thing, which was to draw together those various strands of thought.

Now, pardon a homely metaphor: The theoretical fruit was hanging high in the tree, ripe and ready for picking, and I happened to be in the orchard at the top of the ladder (good imagery for Fresno, and it comes naturally to a farm boy from Delano). My point is that possessing the particular combination of infor-



mation and opportunity I had, almost any respectable social psychologist could have written that attribution paper. (This point is also suggested by Ken Ring's and Daryl Bem's subsequent comments, which implied that a similar synthesis was close to the surface of their thinking.) Again, Heider's friendly spirit smiled on me. I should add that the Kelley cube was included only at the last minute, as a visual aid for the lecture. If I had relied entirely on words, as I had originally intended, people wouldn't have had the Kelley cube to play hacky-sack with all these years.

To back up a little, until it was discouraged by Cronbach's critique of the methodology, much of the earliest work on person-perception after World War II was concerned with the *accuracy* of judgments of other people. Then, following leads in Asch's early work, there came the extensive study of judgments of nouns and adjectives, as illustrated by Norman Anderson's work and Charles Osgood's monumental studies of the meaning of concepts.

The attribution approach was different from the earlier work in that it avoided issues of accuracy and from the later work in that it dealt with interpretations of behavior rather than adjectives. The new questions concerned the causal explanation for the behavior—whether due to the individual and, therefore, informative about that person or due to the group or situational context. Equally important about the attribution perspective was that it was clear that such attributions make a difference. This was shown in the work on misattributions (inspired by Schachter's work on labeling of arousal) and, soon, in Bernie Weiner's studies of affect, moral judgments, and behavior in relation to person-situation attributions.

The major impact of the ANOVA (analysis of variance) model was not in its direct use but in the broader questions it stimulated. Raised first by Leslie MacArthur's research, these questions concerned biases in the use of the covariance information and in the tendency to make "person versus situation" attributions. Those issues came to the forefront in the attribution book I coedited (Jones et al., 1972)—the "orange" book.

Schachter was, in some ways, the friendly spirit responsible for that book. When I happened to be in New York, he suggested that I invite Dick Nisbett and Stuart Valins to come out to UCLA to discuss attribution problems. It was natural and easy, the executive secretary of the appropriate NSF review panel at that time being Kelly Shaver, to get funds for a workshop on attribution. I also invited Ned Jones and two other UCLA participants—Bernie Weiner and Dave Kanouse. We met at UCLA in August 1969 and continued a bit later at Yale. Again, my (i.e., our) good luck held, and the attribution book was quite influential. You may remember that it was printed on beige paper, for which we must credit Hurricane Agnes, which in the summer of 1972 produced floods in Pennsylvania that reduced the printer's supply of paper to that lovely creamy stock.

Most directly traceable to that workshop and most notable in its influence was the Jones and Nisbett chapter on the actor-observer discrepancy, the hypothesis

being that actors tend to attribute their behavior to the situation but observers tend to attribute it to the actors. The latter became the focus of much of Ned's subsequent work on insufficient discounting (or "correspondence bias," as he called it) and the basis of work that led to Lee Ross's famous concept of the "fundamental attribution error." (So we see that questions of accuracy crept back into social perception work after all.)

### The Individual from the Group

This third counterposing of "individual" and "group" concerns how individual differences are related to—derived from—the group. Here I want to describe my own shift in attitudes toward individual difference and personality research and how the Thibaut and Kelley analysis of interdependence patterns became a platform for analyzing how individual differences are defined and shaped by interdependence.

As I remember it, in the early days we experimentalists were rather supercilious in our attitudes toward colleagues who used personality measures and studied individual differences. We were "real" scientists, using the experimental method, drawing firm conclusions about cause and effect, and not fooling around with mushy correlational data. (A not uncommon and hypocritical exception was made when we used "take measures"—a highly relevant and contemporaneous individual difference assessment—to sort our experimental subjects and "clarify" our experimental results.)

I shared that attitude. Yet individual differences played a crucial role in one of my best studies—with Tony Stahelski—on cooperators versus competitors, their interaction in the Prisoner's Dilemma game, and the behavioral assimilation of the cooperators to the competitors. In their interplay, cooperators quickly begin to act like competitors, but this assimilation goes unnoticed by the competitors and serves only to substantiate their misanthropic beliefs that almost all people are, deep down, competitive like themselves.

That was a nice model. And it was a first step in bringing together my interests in interdependence and attribution—interests I had previously tended to keep separate. Not long after, I tired of laboratory experiments with gamelike tasks and turned to using questionnaires to study real dyads—young couples in love and in ongoing relationships. My first interest was in whether we could extract from their reports the two-by-two outcome matrices latent in the problems they encountered in their lives. The answer is, "Well sort of—but two-by-two matrices aren't quite adequate for the job," that is, the job of describing natural interpersonal situations. Far better descriptions are provided by transition lists, presented in my 1984 paper.

In the course of that work, it became clear that people's satisfactions/dissatisfactions with each other are greatly influenced by the general interpersonal dispositions they attribute to each other. So "attribution" shifted from a peripheral



to a central position in my work on interdependence. It began to make sense to think of people as being outcome-interdependent not only in their actions but also in their attitudes and dispositions.

That view of interdependence was reflected in Thibaut's and my 1978 book. With an advance from Wiley, we moved our families to Morelia, Mexico, for a month, and then quickly lost interest in the original plan, which was to write a revision of our 1959 book. Instead, we worked on two new ideas: (1) a thorough analysis of the domain of two-by-two outcome patterns—to identify all the major problems, opportunities, dilemmas, and so on that such situations present to interdependent people; and (2) a causal model of behavior in such situations. That model distinguishes between the underlying ("given") situation and the transformed ("effective") situation. The latter reflects the new situation created by the attitudes they bring to bear on the concrete problem—such attitudes as cooperativeness, fairness, and dominance. This is a systematic, logical way of identifying and distinguishing the individual differences that are relevant to interdependent life. We were heavily influenced in that elaboration of the theory by Chuck McClintock and his colleagues' work on social orientations—work that continues to be very useful in its explanation and prediction of social interaction.

I might note that in writing the 1978 book, John Thibaut and I became increasingly aware of how greatly our theory depended on various key ideas from Kurt Lewin's writings. Those ideas include, for example, interdependence, contemporaneity, taxonomy of situations, cognitive restructuring of the field, goal conflict, motivational properties of conflict, and (in transition lists) locomotion through a "space" defined by paths and goals. The relation of our theory to Lewin's had been questioned by various commentators over the years, and some writers had even regarded us as disloyal renegades from the Lewin camp. Our experience in belatedly appreciating our theoretical indebtedness to Lewin piques my interest in the recent resurgence of attention to Lewinian "field theory," as evidenced by the very active Society for the Advancement of Field Theory (with much leadership from two other MIT fellow students, Kurt Back and Albert Pepitone); Ross and Nisbett's use of Lewinian ideas in their 1991 book, *The Person and the Situation*; and the 1996 Society of Personality and Social Psychology (SPSP) symposium on Lewin held in San Francisco.

In our 1978 book and my 1979 *Personal Relationships* volume, our "group" focus merged with an "individual" focus. In a sense, we became able to look into both mirrors at once, though for our purposes the group mirror was the primary one, the images in the individual mirror being closely coordinated with those on the group side. In brief, the Thibaut and Kelley theory expanded to include a psychology of individual differences. So I now see a basis for creating tight theoretical linkages between social and personality psychology. The idea, expressed in the 1982 Cartwright symposium paper and developed somewhat further in two recent papers (Kelley, 1997a, 1997b), is to derive logically the relevant personal dispositions from the problems and opportunities presented by situations. Accordingly, the dis-

positions a person is likely to have are a function of the sample of interdependence situations that person has experienced and of the pattern of tendencies the person has been able to "negotiate" with the various partners in those situations.

My research on young couples naturally led to contact with other social psychologists working on relationships. Another brief story concerns the increasing involvement of social psychologists in the close or personal relationship field, that is, in work on love, jealousy, commitment, arguments, divorce, and so on. The pioneers in this involvement were Elaine Walster and Ellen Berscheid. Despite encountering considerable prejudice against the scientific study of "personal" phenomena, in the 1970s they published impressive research on feelings of fairness, interpersonal attraction, and love. At a conference at Vanderbilt, John Harvey suggested to Berscheid and me that a group should be assembled to write a broad-gauge book for the field of close relationships. Again, NSF supported a workshop at UCLA; we assembled a cadre of nine fine social, clinical, and developmental psychologists; and after some Sturm und Drang published *Close Relationships* (Kelley, 1983). In the meantime there were the beginnings of interdisciplinary organizational activities in that field. A signal event was a 1982 conference on relationships at Wisconsin, arranged by Steve Duck and Elaine Walster. That was the first in a series of meetings that evolved into an ongoing international organization, the International Society for the Study of Personal Relationships (ISSPR), which brought experimental social psychologists into contact with researchers from sociology, family studies, and communication. Partly through the ideas in *Close Relationships* and through the authors' participation in that organization, the influence of social psychologists—their theories and their methods—have become diffused through what has come to be known as the "personal relationship" field. Again, the friendly spirit smiled on us.

### The Individual Against the Group

The broad question here concerns when a group member can stand up against the group, maintaining independence of behavior or belief while still retaining membership.

The Festinger program on cohesiveness and pressures toward uniformity (with Schachter and Kurt Back) emphasized the effectiveness of groups in bringing their members into line. A similar emphasis on the power of the group setting was present in many of the key studies in the 1950s and 1960s—those by Solomon Asch, Stanley Milgram, Theodore Newcomb, Muzafer Sherif, Philip Zimbardo, and others.

Several of my studies at Yale raised questions about when an individual might resist those pressures, for example, by being highly valued in the group or by having strong direct evidence from his or her own senses. A similar counteremphasis existed (implicitly) in the risky shift studies and (explicitly) in Moscovici's work on minority group influence—the influence of an initially divergent minority.



The same issue was raised later in my ANOVA model, which explicitly counterposed group versus individual information sources, in the form, respectively, of the consensus versus consistency criteria. One of my favorite studies, with John Harvey, provided a neat experimental demonstration of the effect of informational consistency on confidence in one's judgment—something we demonstrated experimentally where before it had been indicated only by correlational evidence.

The possible behavioral independence of a person was, of course, one of the major questions for interdependence theory. The (logical and perhaps mundane) generalization was that behavioral independence is possible when you are less dependent on others than they are on you. This generalization proves to have implications for a variety of basic events in close relationships, such as who has the most say in its affairs, who is most free to deviate from its norms, who is least likely to worry about being left by the partner, and who, indeed, is most likely to leave.

Interdependence, Lewin's criterion for a real "group," comes in several forms. The Thibaut and Kelley 1959 book focused on outcome interdependence, but as our second, 1968 *Handbook* chapter emphasized, social psychology also yielded a great deal of evidence about information interdependence, for example, in the Bavelas-inspired communication network studies, the sharing of information in Elliot Aronson's jigsaw classes, and recent studies of jury decisions.

It has now become clear to me that the analysis of the interdependence between an individual member and the group requires even further differentiation. They are interdependent in their concrete outcomes, but they are also interdependent in how they use their outcome control (as in being cooperative or competitive, altruistic or selfish). They are interdependent in their control over movement into, through, and out of situations, but they are also interdependent in how they use those controls (as in being a leader or follower, active or passive). Similarly, they are interdependent in the information they control (i.e., to which they have access) but also in how they use that information (in their attentiveness, carefulness of analysis, etc.). Particularly important is their interdependence in the communication of information (as in being open, honest, and trusting or secretive, deceitful, and suspicious).

These comments are not meant to overwhelm the reader with the obvious but merely to highlight the multidimensional nature of the relation between the individual member and the group. At each of these nexuses of interdependence, the member has power over the group and they over him or her. (So it is not surprising that my distinctions among components of interdependence begin to resemble Bert Raven's very useful distinctions among various bases of power.)

For the question at hand, of when a member can stand up against a group, these distinctions suggest that the social influence effects from the earlier work (by Festinger, Asch, Milgram, and others) are not subject to simple interpretations. Over the years we have been much impressed by those results and have placed various dramatic interpretations on them. However, I believe that we do

not yet know what mix of factors separate the conformers from the nonconformers under the various conditions. The utilitarian, coordinative, solidarity, ethical, reality, and self-regard factors in most acts of conformity versus resistance are, in my judgment, quite complex. Given this list of factors, it is not surprising that, for example, the subjects in Asch's line-judging experiment were deeply disturbed by discrepancies between their own and their fellows' judgments. That disturbance surely reflected, in part, their puzzlement about reality considerations. But they must also have been perplexed about possible concrete rewards and costs, group incoordination, demonstrating "good membership," the ethics of their fellows, and the consequences of their verbalizations for their self-regard. This multidimensional perspective leads me to warn against blithely oversimplified and, too often, cynically misanthropic interpretations of conformity.

### Concluding Comment

By now the reader will probably have been overdosed on the friendly spirits and my stories. However, in these comments I have not exaggerated my feelings about how and why my career proceeded as it did. It is clear to me that the course of my work and the roster of people I've worked with have been influenced very much by various chance events and timely opportunities. Perhaps the stories suggest that the *process* of my work—the interactions, meetings, working groups—are more salient in my memories than are the *results* of that work. That is not entirely correct. I have decided not to use this occasion to lay out the cumulative results of the theoretical work that John Thibaut and I began and that I continue to this day. In that regard, I'm hoping that the benevolent causal structure of my world will continue to be what it has been in the past and that there will be a few more smiles from the friendly spirits. But that is to challenge fate, and I do better to wish for the spirits to smile on the future of social psychology.

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### 3 A Social Psychologist Examines His Past and Looks to the Future

Harold B. Gerard

Harold Gerard begins by pondering the succession of events that determined the direction of his career, recalling the prominent role that happenstance often played at critical junctures along the way. He reviews his work at Bell Labs and in academia, ranging from explorations of the dynamics underlying the Asch paradigm to his collaborations with Ned Jones (which led to their well-known text on the foundations of social psychology text), to his work on dissonance theory, to his large-scale investigation of the effects of school desegregation in the Riverside, California, school district. He reviews his “last major effort working in mainstream social psychology”—his study with Orive at UCLA, published in 1987, on the dynamics of opinion formation based on dissonance and social comparison theories. After these studies, Gerard became increasingly involved in psychoanalysis. He tells how and why this came about and mentions his recent attempts to apply the methods of experimental social psychology in exploring psychoanalytic dynamics in early mental development. He argues for the crucial importance of psychodynamics in understanding social interaction, concluding with the hope that social psychology will return to the “natural connection between personality and social behavior.”

The cognitive revolution in psychology, which was instigated in part by social psychologists beginning in the late 1950s, has all but eliminated concern with the psychodynamics that underlie how we perceive and behave toward others. Nearly fifty years ago, when Bert Raven, another contributor to this volume, and I were in the graduate program in social psychology at the University of Michigan, our reading list for the social psychology preliminary exam included much in the way of psychodynamics. We read Sigmund Freud, Anna Freud, Karen Horney, Erich Fromm, Clara Thompson, Harry Stack Sullivan, Heinz Hartmann, Ernst Kris, and others. Both Theodore Newcomb and Daniel Katz, the senior faculty in the program, held the conviction that the personality dynamics studied by psychoanalysis